The Secret of the Brownian Movements.

By R. Meade Bache.

(Read before the American Philosophical Society, April 20, 1894.)

We first hear systematically of molecular movement, as a thing directly visible, from the writings of the distinguished botanist, Dr. Robert Brown, embodied in a paper entitled, "A Brief Account of Microscopical Observations, Made in the Months of June, July, and August, 1827, On the Particles Contained in the Pollen of Plants; and On the General Existence of Active Molecules in Organic and Inorganic Bodies."

Of course we know inferentially, through physics and chemistry, that the particles of bodies move under the influence of various extraneous forces, and we know, too, that, in the case of matter in a gaseous form, they move also under the influence of force inherent in their own constitution. But the movements of which I am about to speak relate to the visible behavior of what should be termed inert matter, if that expression were not begging the question of whether or not it has in itself power of motion, and thus deciding it in favor of the view that it has not, but is moved by extraneous force. We must begin, therefore, as is but right, by assuming that it is an open question as to whether or not certain particles, in aqueous suspension, call them molecules or otherwise, as one pleases, organic or inorganic, but not endowed with animal life, are of themselves capable of movement, or are moved by some extraneous force or forces at present unknown. The solid ground of fact from which we start is that, under the conditions mentioned, they do move, for that is undeniable and admitted.

Dr. Brown soon discarded, through disproving in the course of his investigations the surmise which he had made, that the particles of pollen indicated by their motion a mode of function analogous to that of spermatozoids, and rested eventually in the conclusion that particles moving in aqueous solutions are not confined to organic bodies or to their products. He had conducted a series of experiments on finely crushed glass, on simple earths and metals, with many of their combinations, and rocks of all ages, including those in which no organic remains haye ever been found. Moving particles presented themselves in each of the constituent

minerals of granite. Even a crushed fragment of the Sphynx gave the same results. He tried substances of both aqueous and igneous origin, travertine, stalactites, lava, obsidian, volcanic ashes, meteoric matter, manganese, nickel, plumbago, bismuth, antimony, arsenic, asbestos, actinolite, tremolite, zeolite, and steatite. He tried particles of wood, living and dead, linen, paper, cotton, silk, wool, hair, and muscular fibre that had been exposed to fire under the blowpipe, douched with water, and submitted to immediate examination. The particles from all these substances exhibited as vivid movement after as before they had been so treated.

Dr. Brown, about twelve months later, went on further to declare, under the head of "Additional Remarks on Active Molecules," that :—"The result of the inquiry at present essentially agrees with that which may be collected from my printed account, and may be here briefly stated in the following terms; namely, that extremely minute particles of solid matter, whether obtained from organic or inorganic substances, when suspended in pure water, or in some other aqueous fluids, exhibit motions for which I am unable to account, and which, from their irregularity and seeming independence, resemble in a remarkable degree the less rapid motions of some of the simplest animalcules of infusions. That the smallest moving particles observed, and which I have termed Active Molecules, appear to be spherical, or nearly so, and to be between 1-20,000 and 1-30,000 of an inch in diameter; and that other particles of considerably greater and various size, and either of similar or of very different figure, also present analogous motions in like circumstances. I have formerly stated my belief that these motions of the particles neither arose from currents in the fluid containing them, nor depended on the intestine motion which may be supposed to accompany its evaporation. These causes of motion, however, either singly or combined with others,—as, the attractions and repulsions among the particles themselves, their unstable equilibrium in the fluid in which they are suspended, their hygrometrical or capillary action, and in some cases the disengagement of volatile matter, or of minute air bubbles,—have been considered by several writers as sufficiently accounting for the appearance. Some of the alleged causes here stated, with others which I have considered it unnecessary to mention, are not likely to be overlooked or to deceive observers of any experience in microscopical research; and the insufficiency of the most important of those enumerated, may, I

think, be satisfactorily shown by means of a very simple experiment." [Dr. Brown here alludes to what he details at considerable length, as to the trituration together of oil and water, so as to secure, in one case, by a large proportion of water to oil, lacunæ of water of various sizes, filled with particles, protected from evaporation by the oil, and in the converse case, by a small proportion of oil to water, to secure minute oil-drops on the surface of the water, some of which drops were not larger than the solid particles in the water.]

These passages from the writings on this subject of so skillful, careful, and conscientious an observer as Dr. Brown was, excite regret that he had not pursued the subject further. He would have found, among other things, a much simpler and more lasting method of excluding air from the drop under observation, than that which he adopted in forming lacunæ of aqueous fluid protected from evaporation by immersion in oil. He would in all probability have discovered the real cause of the motion of organic and inorganic particles in aqueous fluids. But he dropped the subject, perhaps because he was obliged so to do on account of the pressure of other research, when he had obtained some valuable results, leaving his investigations negative as to conclusions. He doubtless began with the supposition, which seems common at first to all investigators of the subject, that evaporation might represent a shock that would move the particles suspended in water; that vibration from mechanical sources might account for the movements; that currents set up in the drop of water by differences of temperature in it or slight differences in the temperature of the air surrounding it, might account for them; or that mutual attractions, derived from gravitation, and inherent in the relative density of the particles themselves, might do so; and he found, as every one else will find who experiments in these directions, that the movements go on independently of currents and independently of heat, generating or not generating currents, independently of light, and in the case where the particles represent an extremely fine division of matter, and are at the same time of low specific gravity, even independently of terrestrial gravitation; and without relation to their specific gravity, with the force of their mutual gravitation entirely masked.

Some of these things Dr. Brown implies that he saw in the course of his investigations; others he could hardly have failed incidentally

to see, but one that I have mentioned, he did not rightly interpret. It was a distinct lapse in his acuteness of observation, that involved in his mention, without perception of its deepest significance, of minute drops of oil, some of them not exceeding the solid particles themselves in size, standing nearly or altogether at rest on the surface of water; the pointing of which fact is very clear, one which, had it been known to me, would have induced me to try oil as well as water among the first instead of among the last of my own experiments, as actually happened in their sequence.

To afford facility for the fullest comprehension of the subject, it becomes proper here to resume the historical tenor of our way with a brief account of some of the views of Herren Wiener, Exner, and Schultze, more recent than those of Dr. Brown. In order to avoid the responsibility for the necessary condensation, I prefer to quote, as follows, from the summary of their views in the *Jahresbericht* of 1867:

"Then Chr. Wiener, from whose account the preceding historical remarks are quoted, instituted microscopic observations of these movements, and came to the conclusion that this trembling, irregular, unsteady motion of solid molecules, which alter their direction in the briefest fraction of time in their zigzag course, has for its basis the continual movements which, by virtue of their molecular constitution, belong to fluids. He learned through his investigations (1) that the movements are not those of infusoria; (2) that the movement is not communicated to the fluid; (3) that the trembling movement is not in any way derived from the varying attraction and collision of the various oscillating molecules with one another; (4) that the movement is not derived from changes of temperature; (5) that, also, the movement is not derived from evaporation. Consequently, there remained to him nothing to ascribe as the cause of the peculiar movements but the property of the fluid itself. This explanation received direct confirmation from Wiener's observation, that the amount of the movement has a certain relation to the size of the molecule. Lately S. Exner has extended the investigations of Wiener. Among the various influences which Exner sought to test with reference to the molecular motion was whether either chemical causes or mechanical ones, such as pressure, vibration, and so forth, could in any way produce an acceleration or a retardation of the effect. Only by exposure to light and heat did the motion become accelerated, and then in such a manner as,

in the case of glycerin, the molecules of which under ordinary conditions show scarcely any, if any movement at all, to exhibit it clearly when warmed up to fifty degrees centigrade of temperature. Exner also examined into the properties of fluids in which solid molecules remain suspended. The results of his investigation resolve themselves into the following points: (1) The liveliness of the molecular movement is heightened by light and heat, and by radiant as well as by conducted heat; (2) one of the consequences of the molecular movement is, that the molecules, in a specifically lighter fluid, not only do not sink to the bottom, but overcome the force of gravitation to such a degree as to spread themselves equally throughout the fluid; (3) the velocity of this scattering is as the intensity of the molecular movements influenced by light and heat. It should be mentioned here that Fr. Schultze had already stated that substances, when most finely divided, especially such as seemed under the microscope to be amorphous, and exhibited the brownian movements, remain suspended in pure water and in many other fluids for days, weeks, and months at a time, so that the fluid containing them presents a cloudy, or at least an opalescent appearance."

The account of the views of Herr Wiener in the *Jahresbericht* makes an important omission. It devotes itself chiefly to reciting what, in his view, does not cause the brownian movements, but does not mention precisely to what he does ascribe them. Herr Wiener says, in the last paragraph of his article, in *Poggendorf's Annalen*, 1863:—"The weight of the preceding conclusion, that one cannot ascribe the trembling movement to any exterior cause, is very greatly added to by the ascertained fact, that the diameter of the similarly moved water masses is so small that it nearly corresponds with the wave-length of red light, and still more closely with that of radiant heat." This passage gives the keynote to his views on the brownian movements. After a most elaborate series of experiments, in which he measured on a micrometrically divided glass slide, with the addition of diagonal lines, and by watch, the range and the time of the movements, he reached his most important conclusion, that, because the dimensions of the aforesaid wave-lengths of light and heat have a certain close correspondence with the diameters of the minutest particles and water masses, they form the moving impulse of the motions of the particles. He pictured to himself that the æther surrounding the particles, being continu-

ous with the æther of space, acts, through the rays of light and heat on the particles and minute water masses, generating in their interaction, as the visible resultant of the forces in play, the movement of the particles in suspension in aqueous solutions. But, if the cause of the movements assigned by Herr Wiener were the true one, the same cause ought to be operative in the case of alcohol and in that of the fixed and volatile oils: but it is not. This conclusion of Herr Wiener's seems to me to be derived from the unsatisfactory fact of a coincidence, of which kind of proof we habitually perceive more than enough to obscure, bewilder, and often to baffle our feeble efforts to penetrate beyond the veil of phenomena, of things as they seem, to the everlasting noumena, things as they are, near the inscrutable throne of nature.

I will not weary my hearers with the recital of the numerous details of my own experiments, the names of the substances that I tried, the modes in which they were treated, the manipulations of various sorts necessary to the prosecution of the work. Every one knows the difficulties that will arise in new investigations, which will themselves suggest the means of countervailing them as the work proceeds. In this particular case one difficulty was to obtain finely enough divided matter in other liquids besides water. It may be interesting to mention that I did not read anything on the subject until my own experiments were nearly finished. By this course I avoided any possible bias expressed or implied in the directions to be pursued and the conclusions to be drawn, and I had ultimately the satisfaction to perceive, as I had often before observed, how, owing to the constitution of the mind, men necessarily follow the same general and often particular track in their procedures. It is not in the course they follow, that they differ much, but in the conclusions which they reach in pursuing what is virtually the same way. Fortunately for me, constrained to be absent for months in the field on geodetic duty, and at all times constantly engaged at my profession, night still lent itself to my slowly accumulated results. That the investigation was most interesting, I need hardly say.

As electric currents have been demonstrated in the human body, I naturally thought that all slight differences of tension between the liquid and particles, or in the liquid itself, might set up electric currents. Therefore I passed the galvanic current through liquids filled with particles, watching them carefully. There was not the slightest

visible effect thereby produced on the movements of the particles. If the movements had been produced by electric currents, then so strong a current as I often passed through a drop of water ought to have left no manifestation of movement possible from the necessarily weak, if actually existing, currents supposed to be actuating the particles. The molecular movement, so-called, is, as described by Herr Wiener, a zigzag one, but that term does not exactly convey the peculiarity of the motion. It is a combination of a jerky, wobbling movement, performed within determinate bounds, entirely irrespective of the sweep of currents in the liquid, or even of the effects in some cases of terrestrial gravity, and in no case seemingly affected by the influence of local gravitation of particle to particle. Taking the vermilion of the sulphide of mercury, as finely divided as it can be made, and turning the microscope at even a slight angle from the vertical, the effect of terrestrial gravity on the particles becomes at once apparent, but taking the carmine, reputedly made of cochineal, the particles are not affected in the slightest degree by terrestrial gravity. Of course it is hardly necessary to say that any solution should be weak, in order to allow the substances under examination to receive the finest division of which each is susceptible as dissolved. The specific gravity of sulphide of mercury is not only much greater than that of cochineal, but additionally it is not susceptible of nearly so minute division as cochineal is. Of all substances that I experimented with, cochineal seemed to be that which is capable of the finest division, and at the same time of the most brilliant illumination. Gamboge, which appears to be the substance of predilection among many persons to experiment with for the brownian movements, offers nothing comparable to the brilliancy and the fineness of particles of carmine derived from cochineal. With a weak aqueous solution of carmine one may see by daylight, on a background of faint blue, and by ordinary artificial light, on a golden one, thousands of tiny particles, bright as sparks of ruby, shimmering and performing their independent evolutions over the field of view.

Just as one sees a boat managed by an unskillful helmsman pursue its erratic way in going about, being taken aback, or heeled over by a flaw of wind, without for a moment attributing its movements to currents or any other cause but the true one, so the constant observer of the brownian movements knows full well that the particles themselves are moving, not being moved by currents or by gravitation

towards the earth or among themselves. He, from the first, recognizes the fact that the smaller the particles are, the more vivid is their movement. He recognizes another, that, although many large particles do not, as masses, move at all, yet the larger masses are all alive, as it were, with smaller ones, seen clearly around their periphery, on the silhouette of which they are seen plying like banks of oars in an ancient trireme. He is struck with and convinced of still another thing, that whereas one might expect to find that all particles would manifest an attraction for each other through gravitation, and that the larger and largest, but all in proportion to their relative size, would attract and absorb the relatively smaller and smallest ones, nothing of the kind occurs, but the smaller, down to the smallest, go their own way, sometimes even touching the largest and bounding off and away as if they do not, as indeed they do not visibly, submit to the force of gravitation. Of course they cannot escape the influence of gravitation, whether terrestrial or among themselves, but the effect of gravitation upon them is masked, in what manner will appear later.

It seemed to me that magnetic earth-waves might affect particles in such delicate suspension as those of which we are speaking, some of which are no greater in diameter than 1-100,000 of an inch, seen under various powers capable of magnifying from 650 to 1300 diameters. Accordingly, I have placed the particular fluid under examination in the lines of force of a permanent magnet, with the magnet on one side and the keeper on the other of the drop of fluid. Concentrating the gaze on individual particles, to observe if their movement were modified, and then on others in succession, and often repeating the experiment, nothing could be observed other than the movements existing before the magnet had been brought into requisition. The only kind of particles susceptible to the influence of the magnet were those of precipitated iron, but iron is always obedient to the magnet.

Heat I applied in various ways, either irregularly or in an endeavor to distribute it as equably as possible on the glass slide on which the particular experiment was made. Mere currents are set up during the adjustment of temperature from radiation. At the same time one can observe and differentiate the motions due to the brownian movements, the motions along currents, and also the motions from terrestrial gravity, if any, exhibited by particles, if

the specific gravity of the substance be great, and the microscope be set at an angle with the vertical.

Cold I also applied, putting the slides with their cover-glasses in a freezing mixture of broken ice and water, and reducing them to a very low temperature. Still the movements went on as apparently unmodified as ever. Herr Exner says, it will be remembered, that glycerin, which under ordinary conditions shows absolutely none, or almost no molecular movement, shows it clearly when warmed up to the temperature of fifty degrees centigrade. In all the finely divided bodies, however, which I examined, there seemed to be no increase or diminution in the intensity of the movements, corresponding with their alternate subjection to heat and cold. There were occasions in which I thought that I observed acceleration from light, but I always ended by imputing it to the force of imagination, and if it were not justly ascribable to that cause, the fact that it could be so ascribed, is proof positive that if, through the influence of light and heat, any intensification of the movements of the particles took place, it must have been very small. Moreover, the evidence is certainly here, to show that even if the movement were intensified by light or heat, that was the only influence that could be ascribed to them, that light and heat could not be deemed the cause of the movement. And lastly, Herr Wiener's micrometric measurements of the range of movement at different temperatures completely bore out this conclusion.

The theory of Herr Wiener, that the movements are due to the action of the red-wave of light and heat is refuted by the single fact that, as I have proved by experiment, one may interpose at pleasure between the source of light or heat and the particles, either a violet glass or a red glass, without being able to observe the slightest alteration in the movements, either as to their range or their velocity. That is to say, red rays may be either partially excluded or selectively admitted, without diminishing or increasing the liveliness of movement. Hence light can have nothing to do with the phenomenon under discussion, and I have just shown, through the citation of the freezing mixture experiment, that heat can have nothing to do with it.

I have reserved to the very last the discussion of the question as to whether or not the shock, if any there be, from evaporation can have anything to do with the movements, although this was a point that entered into my first investigations. I have reserved it to the

last, because its discussion requires more than the brief space which I have devoted to the previous individual results, and because it leads directly to the conclusion that I have finally reached as to the true cause of the movements. I started out with the conception, which it seems is common to every one, that evaporation might be accompanied with a series of minute explosions, which produce shocks that manifest themselves through the mass of an aqueous solution, in the form of minute movements of finely divided matter held in unstable equilibrium by suspension in the fluid, and that these, escaping cognizance from any ordinary observation, might be visible as such, or in their effects, through the instrumentality of high powers of the microscope. I had come to believe long before, from observation and experiment, that no tremors from mechanical agency or any other, except perhaps from evaporation, could produce the peculiar movements known as brownian, and finally it remained to discover if this or any other intrinsic cause were at work that would account for them.

At this point I encountered an obstacle. My high powers of the microscope were both water-immersion lenses. It seemed, therefore, that even when I had had the drop of liquid under observation, sealed beneath a cover-glass, I might have included, by the use of the water-immersion lens itself, an evaporating surface which might have produced the optical illusion of the movement of the particles in suspension. I proceeded, however, with my experiments, upon the assumption that this, as the event proved to be the case, was not true, and meanwhile procured from Vienna a one-fifteenth dry-lens by Reichert, the highest power of dry-lens that he makes.

I had already obtained for high-power lenses a film of liquid thin enough to be observed through all its strata, free of air within the cell, and protected from evaporation by being hermetically sealed. Any ordinary manufactured cell is too deep, and with all precautions taken contains a little air. On the other hand, the mere cover-glass superposed on a glass slide contains too slight a depth of fluid. I made a cell by using gum-shellac traced in a circlet on a glass slide, which cell, when partially dried, is filled to the brim with the liquid to be observed upon, whereupon the cover-glass is pressed into the yielding gum, thereby expressing the contained air with the superfluous liquid, when the product, dried over night, is fit for use on the following evening. One slide, prepared in this manner and filled with a slightly tinted solution of carmine

from cochineal, had been observed upon by me for weeks, with a one-tenth water-immersion lens, and afterwards, upon the arrival of the one-fifteenth dry-lens, was observed upon without showing any variation in the range and vividness of movement of the particles subjected to examination. I have even covered the whole microscope with a pall of thick, black, woolen cloth, so that not a ray of light could enter it, either through the cover-glass or the eye-piece, and then carefully placing the eye close to the eye-piece, have suddenly thrown light upon the cover-glass, when the brownian movement among the particles was perceived in as active play as ever. I have therefore concluded, from all these experiments, that neither heat nor light, nor electricity, nor magnetism, nor mechanical shock, nor finally evaporation, is operative in producing the movements; in a word, that the particles move uninfluenced by these forces. I am therefore constrained to believe, upon the basis of the information that I have obtained in the manner described, that it is not the particles which are moved by their own energy, or moved by any energy directly imparted to them from outside sources, but that it is the fluid that moves them.

If their own energy moves the particles, we should see them at the same time obedient also to the law of gravitation among themselves, manifested as the resultant of whatever forces are in play, whereas, although they must be obedient to the law of gravitation among themselves, its effects, and generally, as well, those of terrestrial gravity, are so masked as not to be at all perceptible. Now, when we consider how minute all of these particles are, and yet that they move apparently unhindered with such constancy and force, it ought to be apparent, I should think, that they have no self-motive power. However erratic the paths of individual particles may be, the likeness among the movements is extraordinary, so almost identical in every case, varying in greatness of range and rapidity only in inverse ratio to the size of the particle, that we cannot conceive of self-actuated particles so behaving; for relative greatness of size in self-actuated particles ought to coincide with relatively greater, not relatively less, energy of movement; whereas, here the case is reversed. But there are other facts that I have observed through experiment, which also prove what I say. In alcohol, and as far as my experiments go, in fixed and volatile oils, the brownian movements are not observable, and yet the microscope plainly reveals that the movements of foreign bodies in all

these is as free as in aqueous solutions, and I think more so. So molecular movements of solid particles in suspension in aqueous fluids must take place perforce of the constant repulsions of the constituent molecules of the particular liquid present—water. The minute drops of oils supernatant on water, some of them no larger than the particles in the water below, observed by Dr. Brown, as he says, to be almost or wholly motionless, so behave because the molecules of water glide by the molecules of a substance for which they have not even the affinity that would compass opposition. Be the globules of oil on water never so small or large, the molecules of the aqueous fluid glide by them. Whether a small or a large globule of oil be the particle itself on water, there is no movement of the particle. Dr. Brown says none, or almost none. I think that he was mistaken, that there is no molecular movement whatever.

Fixed oils have not the same molecular constitution as volatile oils, nor these the same as alcohol, nor either the same as water. Whatever these differences may signify in various behaviors, under varying conditions, one, among the rest, distinguishes water from the rest and all other liquids. Despite its apparent perfect fluidity, the reluctance of its molecules to move among themselves as smoothly as do those of other liquids among themselves is one of its most evident characteristics. We see this exemplified by the way, long since ably demonstrated, in which a wave is built up from ripples, by the way in which the surf breaks along the shore, and in the ease with which a small proportion of oil in contact with water modifies or subdues its energy. Only recently I steered a boat in Boston harbor between two headlands, between which, and far beyond, white-caps covered the surface of the water, surrounding a placid lakelet of a square mile in area, black by contrast to its white-capped margin, over the surface of which lakelet I was soon smoothly gliding; and this change from turbulent to placid waters was wholly due to the merest film of oil from Boston's great sewer discharging its contents three miles away on the lowering tide from the head-house on Moon Island into the current running towards the sea. I am aware, of course, that part of the calmness described was owing to the fact that the oil lessened the friction of the wind on the water. But that was not the only cause of the calming effect produced by the oil. Oil prevents the friction of parts on the surface of water already in agitation, and thereby quiets the wave already risen. The area which I have just described as a

smooth lakelet had been only a short time before my arrival at the place in precisely the same state of agitation as the surrounding waters. The surface is the part where the wave begins to form, and where it receives constant increments, the wind propagating these, and by impact on the growing wave or billow as a whole, forming and propelling it as a mass, despite its tendency in deep water to oscillate freely in the vertical without translation horizontally. It is easily conceivable that, although particles of oil may, as I have stated, experience no sensible friction when in contact with the molecular movements of water, so almost infinitesimal are they in range, yet that oil forming a film over a large surface of water may, through friction, as an enclosing sheath, tend to quiet the water, and thus impair and gradually destroy its ability to continue the massed effect known as a wave, at the place, the surface, where not only is it generated, but where it most effectively tends to preserve its energy of movement.

Thus, it is not only through its weight that water, when set in active motion, becomes so formidable as we know it to be when in angry mood. It is because, besides the momentum with which it can be endowed through its great weight, it lends itself, through its molecular constitution, to the storage of enormous energy and to the yielding up of that energy reluctantly. Assuming the existence of a sea of oil or one of alcohol, and either in a state of turbulence, and moreover eliminating in imagination the difference in weight between these and water, either in comparison with water equally turbulent would gently come to rest.

The difference between Herr Wiener's view and mine is radical. He speaks of the motion common to fluids as the cause of the brownian movements. But such motion, at least as perceptible through the microscope, does not exist, except in water or in some other liquid in which water is, as I have proved by experiment, a considerable constituent. Then Herr Wiener, although accounting for the brownian movements by hypothetical movements common to all fluids, really makes their causation the vibratory effect of rays of light and heat, to which, he thinks, fluids through their constitution lend themselves. I, on the contrary, show that the molecular motion, called brownian, taking place under all conditions imposable, is a property of water and of water only, and that light and heat have naught to do with producing it, although, as I have admitted, they may possibly act in intensifying it. All that I may

claim to have detected is a phenomenon which reverts to the molecular constitution of water, as to which the moving, solid particles in it concerned in the brownian movement have no more to do than has a current-metre to do with the flow of the stream the swiftness of which it measures. We do not deny that a gas may be essentially pure, and therefore homogeneous, a chemical as well as physical entity, and that, nevertheless, its molecules may have repulsions among themselves: on the contrary, we affirm it. Similarly water, recognized, as it is, as a chemical condition, not a mechanical mixture, has, as here demonstrated, repulsions among its molecules.

When I take into account all that I have detailed, and remember also that these moving particles of which we have been speaking, hermetically sealed under glass, as I have them now under cover-glasses, move indefinitely in time, unmodified in range and velocity, through changes of temperature, through light and darkness, through electricity and magnetism, in the presence of every force to which I have been able to subject them, I cannot but think, when I add that these movements are active in proportion to the fineness of subdivision of the particles, that they are caused by the mutual repulsions of the molecules of aqueous fluids. Did I see a relatively large mass moving as vigorously as the most minute one visible to the eye, I should regard this theory as untenable from that single fact alone, because it would be impossible that molecular action should concentrate effect on a relatively great mass; but when I see, as I do, the largest masses remaining unmoved, and descending in the scale, smaller ones, showing the effects of a faint impulse, and descending further still, others exhibiting sluggish movement, until the sight reaches the smallest particle visible, finding in that the most eccentric and vehement movement of any exhibited, I know then that I am looking at a sea where the little waves dash in vain on the impressionless rocks, barely disturb the floating ships and hulks, but twist and swirl and make frantically dance the little cockle-shells of boats wherever they may happen to be upon the surface; and that, in fine, I am witnessing the molecular movement of this sea in its effort to escape into space. The aqueous fluids, finding no release, as under my cover-glasses, the movements would go on forever; finding it in freedom from confinement, they go on until the fluid which is the condition of their manifestation is in a few minutes dissipated in evaporation.

I must confess that, although every conclusion reached through labor bestowed gives a certain pleasure in legitimate appetite for knowledge gratified, yet this is so far beneath what I had thought might lie hidden under the mystery of the brownian movements, I experience a sense of disappointment. I had thought that this investigation might be one of the paths that lead to the solution of the question whether or not energy is immanent in matter or a thing apart from it. For many years after the beginning of this century nothing fundamental in physics was known beyond the fact that matter is indestructible. It has been learned since, but no longer ago than about fifty years, that energy also is indestructible. It still remains perhaps to be shown that energy is but an emanation and manifestation of matter, reacting on it. Advanced as our knowledge is within a few years as to molecular movement, I had hoped that the investigation of the brownian movements might yield some contribution to molecular theory, and thence lead to a profounder knowledge than we now possess of molecular behavior in the abstract. I am able, however, to claim for the demonstration here no more than that the brownian movements are not the self-movements of finely divided particles in suspension in aqueous solutions, which Herr Wiener had also ascertained, but simply that which he did not ascertain, movements generated by the molecular action of aqueous fluids, instead of being, as he and Herr Exner also thought, in differing form, phenomena due to light and heat. Perhaps even this moderate conclusion may be disputed, but it remains to be disproved.

Obituary Notice of Thomas Mutter Cleeman.

By Frederick Prime.

(Read before the American Philosophical Society, April 6, 1894.)

In the year just past, this Society has been called on to mourn the loss of more than the usual number of its resident members. Of these many were taken in the ripeness of their years with their life's work accomplished; some, however, were still in the full vigor of manhood, with apparently a long career still before them. To the latter class belonged the subject of this sketch.

Thomas Mutter Cleeman was born in Philadelphia, July 31, 1843. In